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Economics, Psychology and

the Unity of the Decision Sciences

Abstract

In recent years, several authors have reconstructed the relationship between 20th century economic theory and neuro-psychological research in terms of a three-stage narrative of initial unity, increasing separation and ongoing reunification. In this article, I draw on major developments in economic theory and neuro-psychological research to provide a descriptive and normative critique of this reconstruction. Moreover, I put forward a reconstruction of the relationship between economics and neuro-psychology that, I claim, better fits both the available empirical evidence and the methodological foundations of these disciplines. In doing so, I argue that recent calls to develop a unified interdisciplinary framework for modelling choice are premised on disputable presuppositions concerning both the domain of economic theory and the relationship between this theory's axiomatic foundations and empirical findings about the neuro-psychological substrates of choice.

Keywords: Economic Theory; Psychology; Interdisciplinary Unification; Rational Choice; Neuroeconomics.

Word Count: 7287

Introduction

The philosophical and methodological literature on the relationship between 20th century economic theory and neuro-psychological research has grown remarkably during the last three decades (see e.g. Earl, 1990, Giocoli, 2003, Hands, 2010, and Hausman, 1992 and 2008). Several authors (e.g. Bruni and Sugden, 2007, Camerer and Loewenstein, 2004, and Lewin, 1996) have reconstructed this relationship in terms of the following three-stage narrative (henceforth, the ‘standard view’). First, we find an early period of *unity*, going approximately from the marginal revolution in the 1870s to the 1910s, during which neoclassical economic theory was grounded on psychological foundations. Second, there is a phase of increasing *separation*, prompted by developments in ordinal utility theory and revealed preference theory, culminated in the 1950s with the elimination of psychological findings, constructs and methods from mainstream economic theory. Finally, the systematic violations of mainstream economic theory documented since the 1950s fostered an ongoing *reunification* between economics and neuro-psychology, which builds on advances in behavioural, experimental and neuroeconomics to provide a unified interdisciplinary framework for modelling choice. The idea is that economists and neuro-psychologists can and should draw on a common set of findings, constructs and methods to model choice (see e.g. Gintis, 2004, Glimcher and Rustichini, 2004, Lewin, 1996, and Rabin, 1998).

In this article, I draw on major developments in economic theory and neuro-psychological research to provide a descriptive and normative critique of this standard view. Moreover, I put forward a reconstruction of the relationship between economics and neuro-psychology that, I claim, better fits both the available empirical evidence and the methodological foundations of these disciplines. I shall argue for three claims of general interest to the philosophers and the practitioners of those disciplines. First, contrary to the standard view, neoclassical economic theory was not grounded on psychological foundations. Second, the standard view significantly overstates economics’ alleged separation from psychology and implausibly downplays the role psychological findings, constructs and methods have played in 20th century economic theorizing. And third, the increasing integration between economics and neuro-psychology does not license the standard view’s claim that a reunification between these disciplines is under way. In particular, recent calls for interdisciplinary unification are premised on disputable presuppositions concerning both the domain of economic theory and the relationship between this theory’s axiomatic foundations and empirical findings about the neuro-psychological substrates of choice.¹

¹ I shall use the terms ‘unification’ and ‘reunification’ interchangeably. Which of these two terms is more appropriate depends on whether economics and neuro-psychology were unified in the first place. If my thesis that these disciplines were not unified is

The paper is organized as follows. *Section 1* outlines the standard view's three-stage reconstruction of the relationship between 20th century economic theory and neuro-psychological research, including recent developments in neuroeconomics. *Section 2* combines historical and methodological considerations to articulate a descriptive evaluation of this reconstruction. *Section 3* puts forward a normative evaluation of recent calls to develop a unified interdisciplinary framework for modelling choice, focusing on these calls' presuppositions concerning the domain and the axiomatic foundations of economic theory.

Before proceeding, three points are worth emphasizing. First, economics and neuro-psychology are far from being monolithic disciplines with immutable boundaries. In fact, different research programs have been regarded as part of economics and neuro-psychology during the history of these disciplines (see e.g. Hands, 2010). This variability complicates the task of reconstructing the interdisciplinary relationships between economics and neuro-psychology, but does not preclude us from accurately reconstructing the relationship between specific research programs within those disciplines. In what follows, I use the terms 'economics' and 'neuro-psychology' to indicate the sets of works within these disciplines that are directly concerned with the modelling of individual choice. This use is deliberately broad to encompass the wide range of works (e.g. psycho-physiological studies of valuation, consumer choice theory and expected utility theory) discussed by the proponents of the standard view. However, I shall make such use more precise whenever my remarks apply to specific subsets of these works (see e.g. *Section 2* on consumer choice theory and *Section 3* on expected utility theory).

Second, philosophers have proposed various conceptions of interdisciplinary unification, which respectively encompass the ontology, vocabulary, and other elements of the involved disciplines (see e.g. Grantham, 2004, and Wylie, 1999). In this paper, I explicate the notion of interdisciplinary unification in terms of findings, constructs and methods. I do so both because these three elements figure prominently in the writings of the standard view's proponents (see *Sections 1-2*) and because consideration of those three elements can yield informative insights about the relationship between economics and neuro-psychology (see *Sections 2-3*). Finally, my aim is not just to evaluate some entrenched conceptualizations of the relationship between economics and neuro-psychology, but also to foster a more systematic understanding of this relationship. Below I primarily target philosophical and methodological issues regarding the relationship between economics and neuro-psychology, without engaging in historical debates concerning the exact timing or the number of stages that putatively characterize such relationship. In particular, I shall refer to the specialized historical literature

correct, then the increasing integration between economics and neuro-psychology may foster at most unification (rather than reunification) between these disciplines.

insofar as doing so helps to contextualize and clarify my philosophical and methodological remarks.

1. Economics and Neuro-Psychology: the Standard View

According to the standard view, neoclassical economic theory was “deeply rooted in psychological theories of pain and pleasure” (Padoa-Schioppa, 2008, 451) and “the psychology of sensation” (Bruni and Sugden, 2007, 147). On this view, introspection constituted a reliable source of evidence to validate the basic principles of neoclassical economic theory (see e.g. Lewin, 1996, and Bruni and Guala, 2001). Moreover, psycho-physiological findings concerning the relation between individual hedonic feelings and external physical stimuli provided additional empirical support for those principles (see e.g. Colander, 2007, and Sent, 2004). For instance, Edgeworth mentioned psycho-physiological findings such as the Fechner-Weber Law as “supporting evidence” for his thesis that “pleasure is measurable, and all pleasures are commensurable” (1881, 59). Jevons went as far as to contend that pleasure and pain “are undoubtedly the ultimate objects of the Calculus of Economics” (1871, III.1).²

By the end of the 19th century, however, the reliability of introspective psychology and the collected psycho-physiological findings was severely questioned (see e.g. James, 1890, vol. II). After the turn of the century, a growing number of authors advocated a behaviourist approach to psychology, which aimed to explain human behaviour (including choice) solely in terms of external physical stimuli and observable responses (see e.g. Watson, 1913). The increasing prominence of behaviourism, combined with economists’ ambition to build their theory of choice on a rigorous evidential basis, prompted several authors to develop “an alternative foundation for the theory of choice [...] free of any psychological assumptions” (Sugden, 1991, 757). This ‘escape’ from psychology, in turn, led to the gradual elimination of psychological findings, constructs and methods from mainstream economic theory by the mid-20th century (see e.g. Giocoli, 2003, ch.2, and Kahneman and Sugden, 2005).

I am not concerned here with providing a comprehensive historical reconstruction of these disciplinary developments. For the purpose of my evaluation, I briefly outline the standard view’s reconstruction of three major building blocks of economics’ purported ‘escape’, namely the ordinalist contributions of Pareto ([1909] 1971) and Hicks and Allen (1934), the development of revealed

² Psychology is not the only discipline that has been claimed to provide inspiration and empirical support to neoclassical economists (see e.g. Fisher, [1892] 1925, for some parallels between utility and the physical notion of potential energy). I mention these parallels in passing since they are orthogonal to my remarks about the relationship between economics and psychology.

preference theory by Samuelson (1938a) and Houthakker (1950), and the axiomatic achievements of von Neumann and Morgenstern (1947) and Savage (1954).

On the standard view, leading neoclassical economists regarded utility as a cardinally measurable magnitude that captures the strength of individuals' preferences between different options. In his *Manuale*, Pareto ([1909] 1971) demonstrated that the results of consumer choice theory could be derived without assuming that utility is cardinally measurable in this sense. In particular, he showed that if one assumes that individuals are able to rank combinations of goods, utility can be given an ordinal measure that captures individuals' preference rankings in the sense that, if an individual prefers one combination of goods over another, then the former yields higher utility (see e.g. Moscati, 2013a).³ Hicks and Allen (1934) endorsed Pareto's aim to make consumer choice theory independent of assumptions concerning the cardinal measurability of utility. At the same time, they criticized Pareto for employing notions (e.g. marginal utility) that were still based on cardinalist assumptions. Moreover, they attempted to construct consumer choice theory without making any assumption concerning the cardinal measurability of utility. To this end, they relinquished the notion of marginal utility and based their analyses on the marginal rate of substitution, which they claimed to be measurable from behavioural observations without making direct reference to utility.

In his 'Note on the pure theory of consumer's behaviour', Samuelson praised Hicks and Allen for eliminating the "unnecessarily restrictive" assumption that utility is cardinally measurable (1938a, 61). Even so, he held that their analysis showed "vestigial traces of the utility concept", and criticized their assumption that the marginal rate of substitution is negative and decreasing as "ambiguous" and "artificial" (ibid., 61-2). To drop off the last 'vestigial traces' of utility analysis, Samuelson developed revealed preference theory, which builds on specific consistency conditions to model consistent choice patterns without making substantive psychological assumptions (see e.g. Little, 1950, ch.1-3). More specifically, Samuelson (1938a) demonstrated that if a complete and transitive preference ordering can be constructed from an agent's choices between pairs of bundles, then this agent's choices satisfy what was later called the weak axiom of revealed preference. Houthakker (1950) extended Samuelson's result from choices between pairs of bundles to choices over series of bundles. In particular, he demonstrated that if an agent's choices satisfy the so-called strong axiom of revealed preferences, then her choices reveal a complete and transitive preference ordering (for a formal exposition of these results and their interrelations, see e.g. Kihlstrom et al., 1976).

³ For the purpose of my exposition, I follow the widespread use of the expression 'cardinal utility' to indicate various measures of utility stronger than ordinal utility. For more fine-grained taxonomies of distinct measures of utility, see e.g. Moscati, 2013b.

Towards the half of the 20th century, the development of expected utility theory (henceforth, EUT) and subjective EUT provided economists with a rigorous axiomatic framework for modelling choice in conditions of risk and uncertainty respectively.⁴ More specifically, von Neumann and Morgenstern (1947) proved a representation theorem stating that if an agent's preferences satisfy specific consistency axioms - namely completeness, transitivity, continuity and independence - then there exists a utility function unique up to positive linear transformations, such that for any two options, the one preferred is assigned higher expected utility.⁵ Savage (1954) built on this result to demonstrate that a complete preference relation that satisfies further consistency axioms can be represented as if the agent maximizes expected utility relative to a unique subjective probability function. These mathematical achievements enabled economists to model consistent choice patterns without making substantive psychological assumptions. This, in turn, ideally completed economics' alleged separation from psychology (see e.g. Giocoli, 2005).

The proponents of the standard view frequently criticize the disciplinary developments that putatively led to economics' separation from psychology. For instance, Lewin contends that economists' adherence to behaviourism "placed economics in a straightjacket" that precluded progress at the interface between economics and other decision sciences (1996, 1313). Similarly, Bruni and Sugden allege that economics' separation from psychology "may have diverted economics from a path of development" (2007, 147). Fortunately, the standard view goes, a series of contributions at the interface between economics and neuro-psychology "reverse [the] fundamental shift" prompted by this separation (ibid., 146). These contributions build on neuro-psychological findings, constructs and methods to foster a gradual reunification between economics and neuro-psychology. The idea is that economists and neuro-psychologists can and should draw on a common set of findings, constructs and methods to model choice (see e.g. Gintis, 2004, Glimcher and Rustichini, 2004, Lewin, 1996, and Rabin, 1998). Three research programs have been claimed to provide especially significant contributions to the alleged reunification between economics and neuro-psychology.

⁴ An agent faces a situation of *risk* when she ignores which outcome will obtain as a result of her choice, but knows both what outcomes may obtain and each of these outcomes' probability of obtaining. An agent faces a situation of *uncertainty* when she ignores not just which outcome will obtain, but also some outcomes' probability of obtaining.

⁵ An agent's preferences are *complete* if and only if the agent is always able to express definite preferences regarding the options she faces, i.e. for any two options x and y , $x \geq y$ \vee $y \geq x$. An agent's preferences are *transitive* if and only if, for any options x , y and z , $(x \geq y \wedge y \geq z) \rightarrow x \geq z$. *Continuity* requires that, if option x is preferred to another option y but is not preferred to a third option z , then there exists a compound lottery over y and z which is indifferent to x , i.e. if $z \geq x \geq y$, there exists $\alpha \in [0, 1]$ such that $x \sim [\alpha y; (1-\alpha)z]$. *Independence* requires that adding a common component to each side of a choice relation does not change preferences, i.e. if $x \geq y$, then $[\alpha x; (1-\alpha)z] \geq [\alpha y; (1-\alpha)z] \forall \alpha \in [0, 1]$.

First, *behavioural economics* purports to “reunify psychology and economics” by improving “the realism of the psychological assumptions underlying economic theory” (Camerer, 1999, 10575; see also Camerer and Loewenstein, 2004). The idea can be explicated as follows. Standard economic theory posits agents having implausibly sophisticated cognitive and computational abilities (e.g. think of the ability to calculate the expected utility of the available options using perfect Bayesian updating with negligible cognitive costs). However, real-life individuals have a number of cognitive and computational limitations, which cause systematic deviations between their choices and the predictions of standard economic theory. Fortunately, psychological findings enable economists to build theories that make more plausible assumptions concerning individuals’ abilities and thereby fit individuals’ choices better than standard economic theory. Therefore, economists should use psychological findings in constructing and revising their theories. As Rabin put it, “the underlying premise of [behavioural economics] is far too compelling to consider it transitory: *ceteris paribus*, the more realistic our assumptions about economic actors, the better our economics. Hence, economists should aspire to make our assumptions about humans as psychologically realistic as possible” (2002, 658).⁶

Second, *experimental economics* vastly increases the array of observational tools and investigative methods available to economists (see e.g. Bardsley et al., 2010, for a review). Experimental studies have been claimed to advance economic analyses in several respects, ranging from discriminating between competing economic models (see e.g. Guala, 2005, and Smith, 1994) to testing the robustness of economic theories’ implications across diverse experimental conditions (see e.g. Loomes et al., 2003, and Smith, 2002). In recent years, experimental studies have been put forward not just to test standard economic theory, but also to develop more predictive and explanatory theories. In this respect, it is worth mentioning the so-called exhibits, experimental designs that generate empirical regularities at odds with standard economic theory and suggest possible ways of revising such theory (see e.g. Sugden, 2005; see also *Section 3.2* for discussion).

Finally, *neuroeconomics* aims to complete the reunification between economics and neuro-psychology by combining these disciplines’ findings, constructs and methods into a unified framework for modelling choice (see e.g. Camerer, 2008). Neuroeconomics expands behavioural economics by measuring and manipulating variables (e.g. neural correlates of decisions) that behavioural economists were formerly unable to observe. Moreover, it expands experimental economics by supplementing the array of observational tools and investigative methods

⁶ Behavioural economics encompasses a vast variety of works, including Simon’s (1955) studies of bounded rationality, Kahneman et al.’s (1982) investigations of heuristics and biases, and Gigerenzer et al.’s (1999) ecological rationality approach. I expand on some of these works and their interrelations in *Section 3*.

employed by experimental economists with innovative brain-imaging and brain-stimulation instruments. The proponents of neuroeconomics frequently claim to advance economic modelling by building more predictive and explanatory models of choice. Some manifest the ambition to develop “a single, general theory of human behaviour” (Glimcher and Rustichini, 2004, 447; see also Glimcher, 2011, 4). This, in turn, is said to “complete the research program [of] the early classics” (Rustichini, 2005, 203) and fulfil Jevons’ and Edgeworth’s ambition to “reground economic behaviour in [...] cognitive neuroscience” (Quartz, 2008, 460).⁷

2. Descriptive Evaluation

As outlined in the previous section, several authors reconstruct the relationship between 20th century economic theory and neuro-psychological research as a three-stage process of initial unity, increasing separation and ongoing reunification. In this section, I combine historical and methodological considerations to provide a descriptive evaluation of this ‘standard view’. I shall argue for three main claims. First, contrary to the standard view, neoclassical economic theory was not grounded on psychological foundations. Second, the standard view significantly overstates economics’ alleged separation from psychology and implausibly downplays the role psychological findings, constructs and methods have played in 20th century economic theorizing. And third, the increasing integration between economics and neuro-psychology does not license the standard view’s claim that a reunification between these disciplines is under way.

According to the standard view, leading neoclassical economists aimed to make their theory’s assumptions “broadly compatible with what were then recent findings in psychophysics” (Bruni and Sugden, 2007, 149; see also Colander, 2007). Furthermore, “neoclassical economics, as practised by [some of its] pioneers, was based on empirical hypotheses about human psychology” (Sugden, 2009, 859; see also Sent, 2004). These observations are correct, but imply neither that “the psychology of sensation was an essential part of economics” (Bruni and Sugden, 2007, 154) nor that “economics and psychology are essentially siblings separated at birth” (Loewenstein et al., 2008, 648-9). In particular, one may consistently hold that some neoclassical economists made substantive

⁷ Calls for interdisciplinary unification target other disciplines besides neuro-psychology. For instance, Lewin advocates unification across economics and sociology on the alleged ground that “we cannot even begin to formulate a more realistic psychological foundation for economics, if we do not recognize the social forces that influence human decision making” (1996, 1320; see also Ross, 2014, ch.5). I gloss over these claims since my evaluation focuses on unificationist calls at the interface between economics and neuro-psychology.

psychological assumptions, and yet deny that neoclassical economic theory itself rested on psychological foundations (see e.g. Hands, 2010, for similar remarks regarding hedonistic psychology). As Fisher put it, economics is not in the business to “build a theory of psychology”, and does not investigate whether the antecedent of choice is pleasure or some other mental state ([1892] 1925, 11).

Indeed, even those neoclassical economists who made psycho-physiological assumptions doubted the prospects of the project to ground economic theory on psychological foundations. For instance, Jevons held that pleasure and pain are in principle measurable in psycho-physiological terms (1871, 4), and maintained that “no apparent limit exists to the success of scientific method in weighing and measuring [...] the phenomena [of the] mind” ([1874] 1958, 735-6). Still, he put forward rather sceptical assertions concerning the prospects of measuring pleasure and pain directly in psycho-physiological terms. As he famously claimed, “I hesitate to say that men will ever have the means of measuring directly the feelings of the human heart [...] It is from the quantitative effects of the feelings that we must estimate their comparative amounts” (1871, 11). That is to say, even the economists who occupy centre stage in the standard view made contentions that do not fit well the standard view’s claim that neoclassical economic theory was grounded on psychological foundations.

What about the separation that, on the standard view, led to the gradual elimination of psychology from economic theory by the mid-20th century? *Prima facie*, the standard view’s reconstruction of this alleged separation may seem to fit both the claims of leading economists of the time and entrenched interpretations of these economists’ works (see e.g. Bruni and Guala, 2001, on Pareto). Moreover, as noted in *Section 1*, a series of mathematical achievements enabled economists to represent consistent choice patterns without making substantive psychological assumptions. These achievements constituted major developments in 20th century economic theory, but should not be taken to indicate that psychological findings, constructs and methods were eliminated from such theory. On the contrary, psychological findings, constructs and methods figured prominently both in works at the interface between economics and psychology and in mainstream economic theorizing. To illustrate this, let us focus on some often-cited proponents of economics’ alleged separation from psychology.

In his works, Pareto remarked that economists have “a great interest in [relying as little as possible on] psychology” (quoted in Busino 1964, xxiv), and regarded the purported elimination of psychological assumptions from consumer choice theory as one of his main theoretical achievements (see e.g. [1909] 1971, ch.2). Still, he granted that economic phenomena can be given psychological explanations, and claimed that “a day will come when we will be able to deduce the laws of the social science from the principles of psychology” (ibid., II, §1). For his part, Robbins ([1932] 1945) repeatedly emphasized that neoclassical economic theory does not rest on any specific psychology and that economists can validate their psychological assumptions via intuition and introspection without performing

psychological experiments. In particular, he adamantly insisted that “the hedonistic trimmings of the works of Jevons and his followers were incidental to the main structure of a theory which [can be] set out and defended in absolutely non-hedonistic terms” ([1932] 1945, 85-6). At the same time, he dismissed behaviourism as a ‘queer cult’, and held that it is often “impossible” to account for economic phenomena “unless we invoke elements of [...] psychological nature” (ibid., 87-8).⁸

As to Samuelson, his ‘Note’ declaredly aimed to drop off “the last vestiges of the utility analysis” (1938a, 62) and develop a strictly behaviourist theory of consumer choice. In the same years, however, he made substantive contributions to the derivation of ordinal utility theory’s implications for demand behaviour (see e.g. Samuelson, 1938b). Moreover, in later works he presented revealed preference theory as the derivation of “the full empirical implications for demand behavior of the most general ordinal utility analysis” (Samuelson, 1950, 369). I am not concerned here with establishing whether Samuelson ultimately relinquished his early behaviourist ambitions (see e.g. Hands, 2008 and 2009b, and Ross, 2009, for a recent debate about this issue). For present purposes, it suffices to note that Samuelson’s later contributions are not easily interpreted in purely behaviourist terms, and that it would be implausible to portray Samuelson as a leading economist whose sole aim was to free economic theory from psychological assumptions. More generally, the point remains that psychological interpretations of economic constructs have figured prominently in 20th century economic theory (see e.g. Guala, 2012, and Hausman, 2012, ch.7-8, on different interpretations of the notion of preference). Furthermore, various applications of economic theory rely on psychological findings and assumptions for their plausibility (see e.g. Lehtinen and Kuorikoski, 2007, on some applications of rational choice theory).

Regarding the putative reunification between economics and neuro-psychology, it is useful to distinguish the *descriptive* issue whether a reunification between these disciplines is in fact under way and the *normative* issue whether those disciplines’ practitioners should promote and implement such reunification. I shall expand on the normative issue in the next section. As to the descriptive issue, the following remarks are in order. Over the last few decades, promising integrative advances have been made at the interface between economics and neuro-psychology. These advances range from the incorporation of psychological insights into standard economic models (see e.g. Tversky and Kahneman, 1992, on cumulative prospect theory) to ongoing attempts to link neuroscientific measurements, observed behavioural responses, and formal choice theory through rigorous operational definitions (see e.g. Caplin et al., 2010, for an axiomatic test of the so-called reward prediction error hypothesis). In recent years, both behavioural and

⁸ These claims are not inconsistent, but illustrate some of the tensions between Robbins’ diverse methodological commitments. For a detailed appraisal of Robbins’ position, see e.g. Hands, 2009a, and Sugden, 2009.

experimental economics have gained wide recognition among mainstream economists (see e.g. Nobel Press Release, 2002, on the Nobel Prize awarded to Kahneman and Smith for integrating “insights from psychological research into economic science” and establishing “laboratory experiments as a tool in empirical economic analysis”). According to some (see e.g. Shiller, 2011), it is only a matter of time before neuroeconomics attains a comparable standing.

In light of these disciplinary developments, one might well maintain that interdisciplinary integration is a widely endorsed regulative ideal, and that the ongoing integrative advances foster significant progress at the interface between economics and neuro-psychology. Even so, the proponents of the standard view have hitherto failed to specify in what sense exactly these advances would constitute genuine *unification* between economics and neuro-psychology, as opposed to *local integrations* between particular research programs within these disciplines. This lack of specificity is problematic, because different authors advocate rather dissimilar (and often conflicting) integrative strategies (see e.g. Berg, 2014, on the proponents of distinct approaches to the modelling of bounded rationality). Furthermore, economics and neuro-psychology differ profoundly in their methodological presuppositions, employed constructs and explanatory aims (see e.g. Fumagalli, 2010 and 2014). These differences, in turn, severely hinder attempts to provide a unified interdisciplinary framework for modelling choice (see e.g. Fumagalli, 2015; see also Ross, 2011, on various differences between economists’ and psychologists’ conceptions of choice).

Regrettably, several authors seem to overestimate the contribution of recent integrative advances to the alleged reunification between economics and neuro-psychology. By way of illustration, consider Glimcher et al.’s conjecture that by combining economic and neuroscientific approaches, neuroeconomists will develop “a methodology for reconciling prescriptive and descriptive economics” (2005, 214). Neuroeconomists may employ descriptive findings concerning the neuro-psychological substrates of choice to inform standard economic analyses (see e.g. Fumagalli, 2013, on neuroeconomists’ attempts to complement choice-based indicators of well-being). Yet, it is highly doubtful that neuroeconomists’ descriptive findings provide compelling insights regarding what people ought to choose in specific situations. As acknowledged by Glimcher, grounding prescriptive conclusions solely on neural findings is unwarranted not just because of current limitations in the reliability and accuracy of these findings, but also because such findings “are explicitly positive in nature” (2011, 412).

3. Normative Evaluation

Suppose, for the sake of argument, that the relationship between 20th century economic theory and neuro-psychological research may be plausibly reconstructed as a three-stage process of initial unity, increasing separation and

ongoing integration. Even so, there are several reasons to question recent calls to develop a unified interdisciplinary framework for modelling choice spanning economics and neuro-psychology. I already discussed some of these reasons in previous articles.⁹ In this section, I address two major challenges that prominent proponents of the standard view put forward regarding the relationship between economics and neuro-psychology. These two challenges target leading authors' conception of economics as a separate science of rational choice independent of neuro-psychology (see e.g. Pareto, [1909] 1971, ch.1; see also Gul and Pesendorfer, 2008, for a recent defence of such conception). More specifically, the first challenge is "to find and to justify a definition of the domain of [economic theory] which is not vacuous [and] is wide enough for economics to have something useful to say about the real world" (Bruni and Sugden, 2007, 171; see also Bardsley et al., 2010, ch.2). The second challenge is "to find consistency conditions for 'rational' preferences, secure enough to serve as the basis for reliable deductive inferences" (Bruni and Sugden, 2007, 171; see also Earl, 2005).

According to the proponents of the standard view, economists have failed to address these two major challenges. In their view, "for most of the twentieth century" limits to the domain of economic theory "were not discussed [and] there was an [unsupported] implicit presumption that the theory was universal in its application" (Bruni and Sugden, 2007, 171; see also Sugden, 2001). Moreover, the preferences of economic agents "were assumed to satisfy strong axioms of consistency [that] were motivated on a priori grounds but not tested against the evidence" (Bruni and Sugden, 2007, 171; see also Giocoli, 2005). I shall critically examine these two claims in points 3.1 and 3.2 below. In doing so, I argue that recent calls to develop a unified interdisciplinary framework for modelling choice rest on disputable presuppositions concerning both the domain of economic theory and the relationship between this theory's axiomatic foundations and empirical findings about the neuro-psychological substrates of choice. This does not *per se* imply that economics is plausibly regarded as a separate science of rational choice independent of neuro-psychology. Still, it challenges the proponents of the standard view to ground their calls for interdisciplinary unification on more plausible philosophical and methodological presuppositions.

3.1 The Domain of Economic Theory

Several criteria have been proposed to demarcate the domain of economic theory during the history of the discipline (see e.g. Backhouse and Medema, 2009, and

⁹ See e.g. Fumagalli, 2011, on the epistemic and pragmatic trade-offs faced by recent attempts to incorporate neuro-psychological variables into economic models of choice; see also Fumagalli, 2015, on prominent authors' divergences as to what disciplines are to provide the basic constructs for interdisciplinary unification across economics and neuro-psychology.

Maas, 2009). Two sets of demarcation criteria have been especially prominent among leading economists. On the one hand, some define the domain of economic theory in relation to the presence or the workings of specific *causal factors*. For instance, Marshall contends that economics “examines that part of the individual and social action which is most closely connected with [...] the material requisites of the wellbeing” ([1890] 1920, 1; see also Mill, 1836, 318, for the claim that political economy investigates the operation of the causes underlying the production and distribution of wealth). On the other hand, others define the domain of economic theory in terms of particular *features of choice*. For example, Robbins characterizes economics as the science that “studies human behaviour as a relationship between ends and scarce means which have alternative uses” ([1932] 1945, 16; see also Weber, [1904] 1949, 65, for the claim that economic motives concern the satisfaction of needs and desires in presence of scarce material means).

These two sets of demarcation criteria point to different definitions of the domain of economic theory. Moreover, economists debated at length about the relative merits of such definitions. By way of illustration, Robbins’ definition was criticized by other economists both for being overly broad (see e.g. Buchanan, 1964, 214) and for being exceedingly narrow (see e.g. Hutchison, 1938, 54) before becoming popular. Indeed, economists’ disagreements were so profound that several authors doubted the possibility of providing a single coherent definition of the domain of economic theory (see e.g. Fraser, 1932). These observations highlight some of the difficulties faced by economists’ attempts to precisely demarcate the domain of economic theory. At the same time, they make it dubious that “for most of the twentieth century” economists did not discuss limits to the domain of economic theory and implicitly presumed that “the theory was universal in its application” (Bruni and Sugden, 2007, 171). To be sure, some economists took their “analytic categories - scarcity, cost, preferences, opportunities, etc. - [to be] universal in applicability” (Hirshleifer, 1985, 53). Yet, even the staunchest advocates of mainstream economic theory typically *argued* - rather than *presupposed* - that their constrained optimization techniques could be applied to model phenomena that were previously deemed to be outside economics’ domain (see e.g. Becker, 1976, on racial discrimination and family relations).

More generally, the two sets of demarcation criteria outlined above point to definitions of the domain of economic theory which seem “not vacuous [and] wide enough for economics to have something useful to say about the real world” (Bruni and Sugden, 2007, 171). This does not exempt economists from the alleged need to test economic theory within the domain delimited by these demarcation criteria (see point 3.2 below for discussion). Still, it challenges the proponents of the standard view to specify why exactly those demarcation criteria would be inadequate. In this respect, it would be overly restrictive to require economists to determine “whether *any* given class of behaviour falls in the domain of the theory, *prior* to testing the theory’s predictions about that

behaviour” (Bruni and Sugden, 2007, 156, italics added). For *in primis*, this requirement presupposes - rather than shows - that an adequate definition of the domain of economic theory must demarcate this domain in terms of predetermined classes of behaviour. And second, economists’ constrained optimization techniques have been recurrently applied to model classes of phenomena beyond those (e.g. human individuals’ choices) traditionally included into such domain (see e.g. Kagel, 1987, on animal foraging, and Glimcher, 2011, ch.6, on the activation patterns of specific neural areas).

3.2 Testing Consistency Axioms

As to economists’ purported failure to test specific consistency axioms against the available empirical evidence, the following remarks are worth making. Economic methodologists debated at length as to how the economic theory of choice fares in terms of specific criteria of empirical testability (see e.g. Blaug, 1992, ch.4, and Hands, 1985, on falsificationist criteria). In this context, several authors criticized economists for displaying “an endemic unwillingness [...] to face up to empirical questions” (Sugden, 2001, 128; see also Earl, 2005, 917). The idea is that although economists frequently *claim* to value the possibility of testing economic theory’s consistency axioms against the available empirical evidence, they do not *in fact* abide by rigorous principles of theory testing. As Blaug put it, economists “preach the importance of submitting theories to empirical tests, but they rarely live up to their declared methodological canons” (1992, 243; see also Sugden, 2008, for similar remarks).

These criticisms invite the following two rejoinders. First, economists are commonly more open to *test* and *modify* economic theory’s consistency axioms than the standard view alleges. And second, many economists’ reliance on specific consistency axioms derives not so much from their purported unwillingness to confront recalcitrant empirical evidence, but rather from their adherence to *justifiable principles* of theory testing. Below I expand on these two rejoinders in turn, focusing on how economists responded to the empirical findings against standard EUT emerged since the 1950s. Standard EUT occupied centre stage in economic theorizing when these findings were initially presented (see *Section 1*). Hence, economists’ responses to such findings constitute an interesting test case for assessing economists’ theory testing practices.

From the 1950s onwards, a wide array of behavioural findings have been claimed to cast doubt on the descriptive validity of EUT’s consistency axioms. These findings putatively demonstrated that individuals’ preferences violate such axioms in a variety of choice settings (see e.g. Aumann, 1962, on completeness, Lichtenstein and Slovic, 1971, on transitivity, and Allais, 1953, on independence). When these findings were first presented, many economists responded by doubting their reliability and robustness. In particular, several authors alleged that the collected findings were more likely to reflect peculiar

features of the examined choice settings than widespread violations of EUT (see e.g. Grether and Plott, 1979, on how learning and incentives tend to reduce some of the reported violations). EUT, however, came under increasing pressure when the systematic character of its violations was documented (see e.g. Kahneman et al., 1982, on the robustness of various violations of EUT to variations in experimental conditions). The systematic character of the documented violations prompted some to challenge not just the descriptive, but also the normative tenability of specific axioms (see e.g. Levi, 1986, on completeness, Sugden, 1991, on transitivity, and Allais and Hagen, 1979, on independence).

Economists developed three main lines of response to these challenges. Some attempted to insulate EUT from alleged disconfirming findings by delimiting the domain within which this theory can be legitimately tested (see e.g. Plott, 1996, on the discovered preference hypothesis, which states that EUT accurately predicts the decisions of agents who had sufficient opportunities and time to discover which actions best satisfy their preferences). Others defended the normative plausibility of EUT's consistency axioms by pointing to the losses one may incur when violating these axioms and to individuals' willingness to revise their choices once they realize that such choices violate specific axioms (see e.g. Hands, 2014, for a review of some such defences). Still others modified the axiomatic foundations of EUT so as to reconcile this theory's implications with observed choice patterns (e.g. Aumann, 1962, relinquished completeness, Machina, 1982, abandoned independence, and McClennen, 1990, relinquished both completeness and independence). That is to say, economists did not uncritically take the collected findings to undermine EUT, but did not dismiss these findings either. On the contrary, they frequently responded to such findings by testing and modifying the axiomatic foundations of EUT.

As to the justifiability of economists' reliance on specific consistency axioms, it is instructive to consider the often-debated issue whether economists should apply strict falsificationist criteria to test EUT's axioms. The empirical implications of EUT are typically conditional upon a variety of *ceteris paribus* qualifications and other auxiliary assumptions (see e.g. Caldwell, 1991). The very act of deriving testable implications from such theory often requires one to introduce auxiliary assumptions about test conditions (see e.g. Smith, 1991). Moreover, economists frequently have lower confidence in some of their auxiliary assumptions than in EUT's consistency axioms (see e.g. Starmer, 1999). This by no means implies that economists "cannot perform [...] controlled experiments" (Samuelson and Nordhaus, 1985, 8), or that "economics must be a non-laboratory science" (Lipsey, 1979, 39). Still, in many choice situations empirical findings contrary to EUT's implications are more plausibly regarded as evidence against some of the auxiliary assumptions rather than evidence against the investigated axioms. Whenever this is the case, economists' reliance on EUT's consistency axioms can be plausibly accounted for in terms of justifiable principles of theory testing rather than a dogmatic commitment to such axioms (see e.g. Hausman, 1992, ch.12, on the so-called weak-link principle; see also

Duhem, [1906] 1954, ch.6, and Quine, 1953, for similar remarks about hypotheses testing in other disciplines).¹⁰

A proponent of the standard view may object that by performing a series of experimental reproductions economists can narrow down the set of potential sources of recalcitrant empirical evidence and test the validity of specific auxiliary assumptions (see e.g. Cubitt et al., 2001, on the adequacy of agents' incentives). This objection is not without merit. Still, only a naïve falsificationist would argue that the availability of empirical findings contrary to EUT's implications *ipso facto* undermines this theory's consistency axioms. In this respect, it is telling that Popper himself emphasized that when a theory premised on the so-called rationality principle fails some empirical test, "it is sound methodological policy to decide not to make the rationality principle accountable but the rest of the theory" ([1967] 1985, 362).¹¹

A proponent of the standard view may further object that in spite of the difficulties involved in testing economic theory, EUT's consistency axioms are empirically testable and often violated (see e.g. Glimcher, 2011, ch.5). In particular, she might allege that the availability of empirical findings contrary to specific consistency axioms makes it incumbent on economists to justify their reliance on these axioms. Suppose, for the sake of argument, that this objection is correct. Even so, the availability of some empirical findings contrary to specific consistency axioms does not *per se* imply that economists' reliance on these axioms is unjustified. For the justifiability of economists' reliance on those axioms depends on a number of epistemic and pragmatic factors, ranging from the availability of alternative frameworks for modelling choice to what goals economists purport to achieve (e.g. predicting individuals' choices, explaining observed choice patterns, articulating a normative theory of choice). By way of illustration, consider again the literature on the violations of EUT's consistency axioms. As evidence of these violations accumulated, many competing accounts of those violations have been advocated (see e.g. Machina, 2008). Furthermore, no single parsimonious theory can plausibly account for the observed violations of the different axioms (see e.g. Harrison and Rutström, 2009). In this perspective, one may consistently hold that various theories outperform EUT for specific predictive and explanatory purposes, and yet insist that the EUT framework remains a valuable benchmark for both descriptive and normative theorizing (see e.g. Hey and Orme, 1994).

¹⁰ Duhem's thesis differs in several respects from the position later advocated by Quine (see e.g. Ariew, 1984). I gloss over these differences since the cogency of my remarks does not hinge upon such differences.

¹¹ Several authors questioned the cogency of Popper's remarks regarding the rationality principle (see e.g. Hands, 1985, Lagueux, 1993, and Nadeau, 1993). I am not concerned here with assessing the cogency of Popper's remarks. For present purposes, it suffices to note that even the heralds of strict falsificationist criteria caution against unreflective applications of these criteria to evaluating the economic theory of choice.

To see this, let us examine the often-made comparison between EUT and cumulative prospect theory (henceforth, CPT), which Tversky and Kahneman (1992) put forward as an alternative to EUT. CPT frequently outperforms EUT in fitting within-sample choice data thanks to the flexibility conferred by its extra parameters. However, such flexibility tends to hamper CPT's out-of-sample predictive performance, and EUT is more robust than CPT against estimation errors in virtue of its simpler functional form (see e.g. Ross, 2014, ch.4). Moreover, CPT does not specify how exactly agents' reference points are to be determined and how they vary across choices. This, in turn, constitutes a critical operational weakness of CPT (see e.g. Harrison and Rutström, 2008, sec.3, for some illustrations). To put it differently, the predictive and explanatory performance of EUT and CPT can vary remarkably depending on what choice problems one targets, and CPT falls short of providing a general-purpose alternative superseding EUT. This point holds not just for CPT, but also for several other works at the interface between economics and neuro-psychology. In fact, prominent neuro-psychological researchers criticize such works for having "little or no predictive power outside of their bounded domains" (Glimcher et al., 2005, 214, on bounded rationality models) and for having "too many interacting parameters [to be] truly falsifiable" (Glimcher, 2011, 120, on original prospect theory).

Conclusion

In discussing the relationship between economics and other decision sciences, several authors caution against the temptation to provide grand claims concerning the relationships between entire scientific disciplines (see e.g. Guala, 2000, and Mäki, 1996). The idea is that interdisciplinary relationships are exceedingly complex and multifaceted to be constrained into a single overarching narrative. I agree with these cautionary remarks in spirit. Still, combining philosophical and methodological considerations can yield informative insights about the relationship between economics and neuro-psychology. In this article, I argued for three claims of general interest to the philosophers and the practitioners of these disciplines. First, contrary to the standard view, neoclassical economic theory was not grounded on psychological foundations. Second, the standard view significantly overstates economics' alleged separation from psychology and implausibly downplays the role psychological findings, constructs and methods have played in 20th century economic theorizing. And third, the increasing integration between economics and neuro-psychology does not license the standard view's claim that a reunification between these disciplines is under way. In particular, recent calls for interdisciplinary unification are premised on questionable presuppositions concerning both the domain of economic theory and the relationship between this theory's axiomatic foundations and empirical findings about the neuro-psychological substrates of choice. This does not *per se*

imply that economics is plausibly regarded as a separate science of rational choice independent of neuro-psychology. Still, it challenges the proponents of the standard view to ground their calls for interdisciplinary unification on more plausible philosophical and methodological presuppositions.

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